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SHORTER ARTICLES AND DISCUSSION

BIOMETRIC ARGUMENTS REGARDING THE GENOTYPE CONCEPT[1]

> Some . . . persons vainly seek by dialectics and far-fetched arguments, either to upset or establish things that are only to be founded on anatomical demonstration, and believed on the evidence of the senses. He who truly desires to be informed of the question in hand, and whether the facts alleged be sensible, visible, or not, must be held bound either to look for himself or to take on trust the conclusions to which they have come who have looked; and indeed there is no higher method of attaining to assurance and certainty.—William Harvey, *Second Disquisition to John Riolan, Jun.*

IN a recent number of this JOURNAL[2] Dr. J. Arthur Harris has published a general attack on the pure-line or genotype concept of inheritance. In the course of this paper its author sees fit to criticize certain work done by the present writer and his former colleague, Dr. Frank M. Surface. These criticisms in the main seem to me, upon analysis, to rest on either a misconception of what our results actually are, or else a lack of understanding of the real facts regarding certain of the biological points involved. If this is the correct view, it means that our presentation of pertinent data has been either obscure or incomplete or both. It is the purpose of this note to endeavor, if possible, to remedy this defect in some degree at least.

The points made by Harris in criticism of the work done in this laboratory may be considered *seriatim*.

1. In a section entitled "Characters which are not Inherited at all can not be Taken to Prove that Selection in General is Ineffective," Harris very cautiously avoids the direct statement that fecundity in fowls is not inherited, but since about one half of the section (pp. 353–356) is devoted to a discussion of the work of the Maine Station on this matter, it is not unfair to suppose that he is of opinion that this work illustrates and exemplifies the dictum which stands as the title of the section. He concludes in a later section that the present writer's investiga-

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[1] Papers from the Biological Laboratory of the Maine Experiment Station, No. 28.

[2] AMER. NAT., June, 1911.

tions indicate "that the high-laying mothers tend to produce low-laying daughters; selection to increase egg production actually decreases it."

Definite numerical data tending to show that fecundity in the domestic fowl *is* inherited has been presented by the writer in the June, 1911, number of the AMERICAN NATURALIST (pp. 321–345). It obviously need not be repeated here. Any interested person may examine the data and draw his own conclusions as to whether "selection to increase egg production actually decreases it," when this selection takes the form of isolating genotypes of high fecundity.

2. The second point made by Harris is against what was intended to be a popular bulletin on sweet corn[3] in the introduction to which the following statement was made: "It is the purpose of this bulletin to give a brief account of the general features of the experiments carried on during the past three years in sweet-corn breeding. Further discussion of the technically scientific results of this work is reserved for later publication." No such publication has appeared.

The criticism of this bulletin is included in a section headed "Improvement for any Single Character can not be Supposed to be Unlimited." The facts are these: In 1907 selection was begun for earliness in an excellent strain of sweet corn. A *marked* gain was made in the quality after one year's selection. No further gain has been made though selection has been continued.

It is Harris's contention that probably after one year's selection the strain had reached its physiological limit in earliness, and that the result obtained can not be urged against the biometric theory respecting selection, nor in favor of the genotype concept. He further gives the reader the impression that Dr. Surface and I regarded these results on earliness as having an important and critical bearing upon theories of selection and inheritance. For the benefit of those readers who do not follow agricultural literature it may be well to quote the final conclusion (numbered 14) printed on p. 307 of the bulletin Harris criticizes:

> No attempt is made at present to discuss the biological basis of the improvement in earliness observed to follow selection for that character in these experiments. *We are inclined to the belief that much, if*

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[3] Me. Agr. Expt. Station, Bulletin 183.

not all, of this improvement is in reality a physiological rather than a genetic or hereditary phenomenon.[4] The whole subject of breeding for earliness is one which needs more critical discussion and experimentation than has hitherto been given it.

The same opinion is expressed in the body of the bulletin (p. 302). How one could take a more emphatically agnostic position than this regarding the interpretation of the results of the selection of maize for earliness is not evident.

In regard to characters other than earliness, such as ear conformation, fineness of grain, yield, etc., which Harris does not mention in his critical remarks, it was and is our conclusion that the results in hand indicated that these characters were not inherited in accordance with the so-called ''law of ancestral inheritance,'' but that our results with these characters gave support, so far as they went, to the genotype concept, thus agreeing with the findings of other students of maize. Whether from too hasty reading of the bulletin criticized or some other reason, Harris has made it appear in his paper that we had earliness in mind in drawing this conclusion. I trust that our position is now clear.

Harris assures us (p. 359) that he has ''no malicious desire to differ'' from us. This it is a pleasure to know for two reasons: one is that a person would dislike to think that he was the butt of malicious and persistent personal attack from a scientific worker; the other is that since there is no intention on his part to be personal and since ours is the *only* work on maize with which he finds fault or feels it even necessary to mention, it must mean that he finds nothing to object to in the only critical, analytical evidence on genotypes in maize which, so far as the writer is aware, has ever been published, namely, that of East and Shull. It is a distinct gain thus to find that there is one body of evidence in favor of the genotype concept which its critics unreservedly accept.

3. It is suggested by Harris that 200 eggs per year represents about the attainable limit of egg production in fowls, and that the reason no improvement was made in our selection experiment was that this physiological maximum had been reached. Here two points need to be made plain. The first is that 200 eggs per year is *not* the physiological maximum of egg production in the domestic fowl. As evidence here the case of a pullet

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[4] Not italicized in original.

which laid over 300 eggs in a year may be cited. While the case did not occur in the poultry yards of this station, there is no doubt as to its correctness. Furthermore, the case is by no means an isolated one, similar records having been obtained under controlled conditions in other parts of the world. This record was made by a cross-bred Orpington pullet in New Jersey. The history of her laying is as follows: She began laying on November 20, 1909. Up to August 25, 1910, she laid practically continuously, producing in that period 275 eggs. She then went broody, and in late September and October moulted. The September record was 13 eggs; the October record 12 eggs, and up to November 9, 6 eggs, making a total production of 306 eggs in 11 days less than a full year.

Further evidence of the same sort is furnished by the report of the last Queensland (Australia) laying competition which has just come to hand.[5] In this competition 150 pullets took part, comprised in 25 pens of 6 birds each. The aggregate egg production of 12 months, from April 1, 1910, to March 31, 1911 (the natural "laying year" in the southern hemisphere), was 31,165, or an average per bird, taking the *whole lot together*, of 207.77 eggs. The three highest pens (of 6 birds each) made average records per bird of 253.33 eggs, 238.83 eggs and 218.67 eggs, respectively.

The second point is that the Maine Station *flock* never attained an *average* production of 200 eggs per bird, or anything like it. But it can not be supposed that a *physiological limit of selection* for production is reached until the *mean* egg production per bird for the *flock* has reached about the physiological limit of the *individual*. The point apparently not clearly grasped by Harris is this: for a long period of years prior to 1898 when the mass selection experiment at the Maine Station was begun, the strain of birds with which it was begun had been under the observation of the man who inaugurated the experiment, the late Professor Gowell. The mean annual production of this flock had been, according to his statement, which there is no reason to doubt, about 125 to 135 eggs per bird. Now there were then known and are now known many flocks of birds which produce an average of 150 to 160 eggs per bird per annum. To attempt to raise the average production of the Maine Station *flock* from say 130 to say 150 eggs per bird per annum by mass selection could hardly by any stretch of the imagination be considered to be approaching the physiological limit of fecundity of the fowl. This is precisely what was attempted during a period of eight years, and was not accomplished. While there is no doubt in all organisms a physiological limit beyond which selection of any kind can not carry the strain or race, such a limit was most certainly never even remotely approached in the course of the mass selection experiment at the Maine Station.

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[5] *Feathered Life*, Vol. 12, p. 450, 1911.

4. Harris emphasizes by the use of italics the fact that the only correlation coefficients between mother and daughter in respect to egg production which I have ever published, while sensibly equal to zero having regard to the probable errors (with a single exception where daughter averages are used), yet deviated in the *negative* direction[6] when they departed from zero at all. Lest some one should attach some importance to this circumstance it may be said that these coefficients to which he refers cover only one year's work (1907–08). Since that time I have calculated the coefficients of correlation in respect to fecundity between mother and daughter and grandmother and granddaughter for the years 1908–09 and 1909–10, for various age classes of mothers, etc. These correlations involve over 1,000 daughters and over 200 mothers. *All of these correlations are sensibly zero,* having regard to their probable errors. Of 11 such coefficients 8 deviate from zero on the positive side and 3 on the negative side. Putting the 1907–08 published coefficients with these it gives, out of 16 coefficients, 10 deviating from zero in the plus direction and 6 in the minus direction. Certainly it is difficult to see how this could be made to indicate that "high-laying mothers tend to produce low-laying daughters."

By way of conclusion it may be said that the writer hopes that what is here set forth will make clear the facts regarding the points criticized by Dr. Harris in the work from the biological laboratory of the Maine Station. So far as concerns his argumentation respecting the genotype concept as a whole, or his personal opinion of the critical value of the work done in this laboratory no discussion will be entered upon by the present writer. Since the merits of both cases rest upon nothing but an accumulation of plain, unadorned facts available to any one's inspection, it seems useless to try to bolster either of them up by the dialectic methods of a lawyer's appeal to the jury.

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[6] Harris implies that this was true of *all* 1907–08 correlations. This is not so, as any one may see by turning to p. 71 of Bulletin 166, where of the five coefficients published, and these are *all* which have been published up to the present time, three are negative and two are positive.

RAYMOND PEARL.

June 21, 1911.

ON THE FORMATION OF CORRELATION AND CONTINGENCY TABLES WHEN THE NUMBER OF COMBINATIONS IS LARGE

IN earlier numbers of this JOURNAL two papers on that useful tool, the correlation coefficient, have appeared. The first[1] explains and illustrates a convenient method of carrying out the arithmetical routine of calculation, while the second, by Professor Jennings,[2] describes a method for obtaining the coefficient for symmetrical tables without the labor of actually rendering the tables themselves symmetrical.

The purpose of this note is to point out a method of preparing correlation tables where the number of combinations is large. Such tables are not infrequently needed. Suppose, for

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[1] Harris, J. Arthur, "The Arithmetic of the Product Moment Method of Calculating the Coefficient of Correlation," AMER. NAT., Vol. 44, pp. 693–699, 1910.

In a note on this method of calculating the coefficient of correlation, Professor Jennings (AMER. NAT., Vol. 45, p. 413, 1911) suggests reduction in size of the moments by designating the lowest grade by 0 and the succeeding ones by 1, 2, 3, ⋯ n. In this he is quite justified. I have frequently used the scheme he suggests during the last several years, but I did not refer to it particularly in my note, and for two reasons. First, I thought the point sufficiently covered by the statement that the rough moments may be taken about any arbitrary point as origin, and by the suggestion that when the range is very great it may pay to use the conventional methods in calculating the standard deviations. Second, according to my experience it is better, whenever possible, to keep the actual values. When one uses a mechanical calculator the arithmetical routine is (after a little practise) not out of proportion to the advantages. Under many circumstances these are very great: (*a*) all the values have a direct biological (physical) significance, (*b*) the means of arrays may at once be obtained for testing linearity of regression, (*c*) tables for different lots of material may be combined or separated at will by merely summing or segregating their moments, and finally (*d*) I shall show in a forthcoming paper how these moments, once calculated, may be of much service in obtaining some of the more difficult correlations.

[2] Jennings, H. S., "Computing Correlation in Cases where Symmetrical Tables are Commonly Used," AMER. NAT., Vol. 45, pp. 123–128, 1910.